**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Observational constraints on entrainment and the entrainment interface layer in stratocumulus" *by* J. K. Carman et al.

J. K. Carman et al.

pchuang@pmc.ucsc.edu

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Thanks to both reviewers for helpful and stimulating comments, questions and suggestions. Below are our point-by-point replies.

Reviewer #2

R: P834-Lines13-25 – Could surface analyses or satellite observations be used in order to investigate some of the differences between boundary layer characteristics for the different flights? For example, is the strength of the sub-tropical high weaker on days where the humidity jump is smaller? Are the cloud characteristics (e.g., cloud fraction, cloud size) noticeably different between these cases?

Based on your prompting, we looked at satellite images for every flight. There's no obvious connection between the moisture jump and the Sc morphology. In all cases, cloud fraction is extremely high since it's pretty much solid overcast. The size of the convective cells also did not seem to be related to the moisture jump.

R: P-836 The contribution of buoyancy fluxes more that 100m below the cloud top to the "boundary layer integral" represents a possibly large uncertainty in this quantity. I believe that it is important to offer some estimate of this contribution. Could the atmospheric profiles from 30 meters to 1000 meters done at the beginning and end of each sawtooth patterned flight be used to make some estimate of the positive buoyancy fluxes that are not accounted for in the entrainment efficiency calculations? While this would not be perfect, it might at least give an idea of the errors in the calculation. In general, some estimate of the uncertainties in the numerator and denominator of eq. (5) (including those due to filtering) would make the calculated values more useful for interpretation.

We agree that not sampling deeper than 100 m below cloud top is a large uncertainty. Normally long level legs are used to estimate fluxes. In lieu of this, we computed filtered flux values from ascents and descents, obviously, but the ability to average over many hundreds of realizations is, we believe, a minimum requirement to have any confidence in the flux profiles. It's true we could use the two full profiles to estimate this, but since a turbulent BL is by nature very noisy (10 min level legs are usually considered a minimum after all!) we think the uncertainty would completely swamp any signal we see.

Our approach is instead to describe what we DO observe rather than what we wished we could have observed, with the idea that models are flexible enough to accommodate the quirks of the observational strategy and data. We hope that by very clearly defining how we compute our efficiency, it is clear to others how they might compute the same quantities that we have.

R: This may also help justify (P838, Lines 14-15), "We have eliminated these cases from further examination due to the potential for large uncertainty in the calculation" which has little justification as currently stated. Could this be impacting the correlation between the "entrainment integral" and the boundary layer integral"? This could directly impact the conclusion in section 3.2.3.

We feel that the justification for (P838, Lines 14-15) is that the one commonality that all of the most extreme efficiency values have is that they are based on a single value with a small magnitude. The uncertainty associated with one small value is likely to be sufficiently large as to not give us much confidence in these calculated values. So we chose to focus on those days where our confidence in the data is higher, although undoubtedly there is substantial uncertainty there as well. We have added the following text to clarify and better justify this point.

R: P827 Line 23 – "...with a range of approximately 3 to 10 K." How are you defining the "top" of this jump?

We weren't necessarily wanting to define an accurate and quantitative "jump" since, as the reviewer is pointing out, locating a "top" is inordinately difficult in many cases. Our point was more to describe approximately the interfacial region. We have edited the text to say:

"A substantial increase in θ_{v} in the few tens of meters above cloud top occurs... with a range of approximately 3 to 10 K."

R: P830 Line 19-23 – "That the filtered flux is only somewhat smaller than model predictions of the total buoyancy flux is consistent with the notion that entrainment tends to be a small-scale event, with typical length scales on the order of 10 to 30 m based on high resolution observations of liquid water and temperature near the tops of stratocumulus (Gerber et al., 2005; Haman et al. 2007)." – Is this because your averages are over a much smaller length scale for which entrainment events would have a potentially larger impact? Please explain.

We were trying to say that our filtering would have less of an impact on determining buoyancy consumption by entrainment since the spatial scales of entrainment are often be similar to our filtering length scale and smaller than the natural length scale of buoyancy production. We've edited to make this clearer:

"The filtered net negative buoyancy flux more closely matches model predictions, which is consistent with the notion that entrainment tends to be a small-scale event, with typical length scales on the order of $\sim$ 10 to 30 m based on high resolution observations of liquid water and temperature near the tops of stratocumulus (). Thus, the filtering will have less of an impact on the computed flux and thereby better match model predictions. "

R: Figure 6 – Needs an y-axis label

Done.

R: P835 Line 5 – "Observations are not able to identify Zmix..." Should Zmix be Zmgd?

No, z_mix was defined by Moeng et al. 2005 as the maximum height to which turbulent eddies penetrate. Since there's no way to measure the intermittent turbulence in the EIL region from aircraft, at least from these observations, we can't estimate this. In the model, this is diagnosed by the release of a model tracer, but that obviously wasn't something we did during this experiment.

R: Figure 14-15 – There is no discussion of these figures in the text. Either remove figures or add discussion.

Done.

Typos and other small corrections:

All the below suggested corrections were done (except for one as noted). Thanks to the reviewer for having such a keen eye.

P819 Line 28 – Should be "...observations by a number of studies..."

P820 Line 6 – Should be "...as one mechanism for the formation..."

P821 Equation 2 - For completeness and clarity, please define all of the variables in equation (2)

P822 Line 1 – TKE was already defined

P822 Line 4 – Remove the word "and" at end of line.

P823 Line 17 – LES was already defined

P823 Line 18 – "the the"

P824 Line 3 – Define CIRPAS

P827 Line 3 – Define F/PDI

P827 Line 28 – Should be "..sphere, to roughly constant values..."

Table 1 – It would be helpful to the reader if the flights shown in Figures 2 – 5 were indicated in this table (perhaps shaded?)

Figure 1 – Needs a legend explaining what the black (aircraft altitude) and blue(LWC) lines are.

P835 Line 9 – Define DMF (or just spell it out)

Figure 7 (and all subsequent figures) – To help the reader, the caption should clearly indicate that the numbers near each point represent the date of the flight. This was not immediate clear to me.

P840 Line 3 – $d(theta\_v)/dz$ should be delta(theta_v)/delta (z) (as in figure 10)

P840, Line 6 – "results" should be "resulting" : this was not changed

P840, Line 13 – Should be "....this qualitative relationship could be used as......"

P840, Line 14 – Should be "...in the STBL"

P842, Line 7 – Remove the second "both" on this line.

P843 Line 19 – "EII" should be "EIL"

P843, Line 25 – "...(within 10m) of cloud top..." should be "...(within 10m) to cloud top..."

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